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ABSTRACT

In this critique, comments and suggestions are offered that might be integrated into future research by Scott, Yates, and Wheeless on the topics of communication apprehension and alternative instructional approaches. These authors suggest, in their paper, that one's level of communication apprehension should be predictive of attitudes held toward, and of satisfaction with, various instructional methods. This paper concludes that while the overall question posed by Scott, Yates, and Wheeless is interesting, the approach and the methodologies chosen for testing the research are weak. (TS)

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A Critique of "An Exploratory Investigation of the Effects of Communication Apprehension in Alternative Systems of Instruction" by Michael Scott, Michael Yates, and Lawrence Wheeless

by

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The Scott, Yates and Wheeless paper introduces a number of important issues for researchers in communication education, They deal with two provocative and meaningful topics: communication apprehension and alternative instructional approaches. Very briefly, they suggest that one's level of communication apprehension should be predictive of attitudes held towards and satisfaction with various instructional methods. While the overall question is interesting, it is regretable that both the approach and the methodologies chosen for testing the research suggestion are weak. I would hope that the following comments will be taken as suggestions that might be integrated into future research on the topic by these authors.

An an initial rationale the authors cite the Bashore (1971) thesis which according to them found that "students suffering from debilitating communication apprehension generally demonstrate lower academic achievement than students who are not apprehensive about communication (p. 1)." This finding supposedly leads the researchers to at least part of their study's rationale. Yet how they got there is questionable. First, Bashore dealt with high school groups while Scott, et. al. plan to investigate college samples. Research has consistently demonstrated however that there are major differences between these groups on predictor variables of academic success (eg., Levin, 1967) One cannot, in short, generalize with any confidence findings from high school populations to college samples. Second, the vast majority of studies dealing with college populations would make predictions opposite to those drawn by the authors of the paper. Introversion, a personality characteristic quite similar to communication apprehension has received a great deal of attention in educational psychology. With virtual unanimity this research concludes that the introverted individual will be more successful in academic pursuits than his otherwise similar classmates high in extroversion (eg., Beach, 1960; Birney & Taylor, 1959; Bloomberg, 1955; Gebhart & Hoyt, 1958; Kerns, 1957; Knaak, 1957; Krug, 1959; Merrill & Murphy, 1959; Phelps, 1957; Travers, 1949). There seems to be no reason for Scott et al to ignore this research. Third, there are major differences conceptually between aptitude, achievement, and ability. I'm sure the authors are aware of these differences, but in this research effort they fail to even mention the possible differential effects each may have on both attitudes and learning. For example, research has also demonstrated that anytime one examines the effects of anxiety on learning they need to block for level of ability (eg., Grooms & Endler, 1960; Klugh & Bendig, 1955; Speilberger & Katzenmeyer, 1959). Finally the authors of this paper seem to avoid the issue of learning, choosing instead to deal with attitudes and satisfaction. At one point they seem to suggest a main effect for apprehesnion on learning; at another they suggest that

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learning may not be affected at all. I wish they had gone further in their examination of previous research; made reasonable hypotheses about learning, and tested these as well. I would suggest that they go to the vast literature in organizational psychology that delves into the relationship between job satisfaction and job productivity. They will find predictable, albeit complex relationships.

When Scott, et. al. deal with communication apprehension they suggest it may have powerful influences on an individual's collegiate career. But nowhere in the literature on apprehension is there the suggestion that fear of communication (any sort of communication) is the sole determinant of college success. Certainly it is important, but so are other variables such as graduation, approval seeking, peer pressure and a host of other relevant, and in the study, unmentioned and uncontrolled variables. This is especially serious when dealing with college samples since there is already a selection factor based partially on communication apprehension operating (eg., Phillip & Metzger, 1973).

On another level we might ask whether the study is really examining communication apprehension as an important variable in the learning process. To this reader it seems more likely that the authors are looking at some sort of apprehension of technology. Indeed when one looks at the reported data there are strong indications of just that. The only significant difference in teaching approach occured between mediated (technological presentations) and the other two, more traditional forms of teaching (written instructions and oral briefings). Along similar lines, Scott, et. al. suggest that no matter what the technology, interaction with an instructor will be necessary in order for students to learn how to use the machine. This is so only to the extent that West Virginia University cannot afford the cost of printed handbooks.

But more distrubing than the lack of any of the above is the problem one encounters when reading the final sections of the rationale. Given a good research idea, based upon interesting questions, and with knowledge that solutions would be beneficial one would expect a rigorous test of the conclusions drawn. Instead, the reader of this report, much to his or her chagrin, will discover that the authors found this impossible to do. Not impossible, mind you, because of the construct, nor to poor thinking; but only because of the apparent institutional set up at West Virginia University. I, for one, would not mind seeing a direct experimental test of the questions raised. This paper does not do this. But all in all, this would not be a major downfall had the paper included even the barest description of the materials taught the specific system used, the experiences of subjects, and the actual teaching procedures used. When dealing with technological variations in teaching it seems important that a research report include technical data so some generalizability might be made. Again, there is none in this paper.

In the first nine hypotheses Scott and his associates suggest differences between high and low apprehensives on attitudes towards methods, content, and general satisfaction levels about the course given different instructional approaches (oral, written, or mediated). Nowhere do the authors provide us a good rationale for believing that there are significant differences between attitudes about methods, content and satisfaction. How satisfied an individual is with a course is

probably highly correlated with how he or she feels about the content and methods. Research on educational evaluation suggest just that there will be high correlations between these different measures. Consequently I see no reason for the multiplicity of hypotheses, not for the seperate data analyses conducted.

The tenth hypothesis suggests that there will be significant differences between high and low apprehensives on number of tutoring sessions. Given the nature of the construct communication apprehension, this seems quite reasonable. On the other hand data was collected during the eighth week of classes before any student could complete the whole semester. Since tutoring was voluntary any expectation of results must be taken with the old college tradition of "cramming" held in mind. But in addition this hypothesis might not support the construct of high communication apprehension as being debilitating given the literature I cited previously about introversion. Maybe your high apprehensive student just doesn't need the tutoring. Consequently, what the hypothesis is testing remains unknown.

The final hypothesis deals with what Scott, et. al. term efficient learning, What exactly that means I still don't know. It was operationally defined as number of modules completed up to the time of data collection. This comes closest to a hypothesis about actual learning but still the avoidance by the authors is noticeable. Regretably the data reported here was collected during the eighth week of a semester. Knowledge claims based upon such data may have little relevance to information gained at the end of the semester. Maybe high apprehensives are more concerned at the end of the semester about coursework than they are at the beginning. Maybe low apprehensives find little social activity during the first part of the semester, study then, and late in the semester give up studying in favor of spring fashions.

Now to the procedures employed by the authors to test their hypothesis. First, the issue of control is tantamount in an experiment such as this. Yet there are few controls mentioned. There was no control made for ability, no control for experience, attitude or motivation, and no controls whatsoever, to insure that all Ss experienced the different treatments. This last deficiency is especially important. Nowhere in the research is there any assurance that all Ss even experienced oral instructions, written instructions, and mediated instructions. Yet they were examined for attitudes and satisfaction on all three. If the construct of communication apprehension is correct, and if there is such a thing as apprehension of technology then there is no reason to assyme that every S participated equally, or even at all, in all three modes of instruction. Better experimental design could have prevented such a question from ever being raised.

I have alreadt noted the question of generalizability. This to me is exceedingly important given the technological future the authors of this paper suggest here and elsewhere (eg., Lashbrook and Scott, 1975) Whenever one discusses a technical procedure specifications should be provided. What Scott, et. al. have done is similar to a report by medical doctors on a new drug without any specifications of the technical composition.

Scott and his associates collected PRCA scores from 300 students during the first day of classes. After that, fitting with previous research procedures in apprehension research, they selected individuals with scores one standard deviation above and below the sample mean PRCA score. The PRCA has been found to have a relatively normal distribution in previous research. Given the initial n we would expect to find approximately 40 to 50 high and low apprehensives each. For some unexplained reason, these researchers only dealt with half that number. How these individuals were selected is never discussed. Whether the selection was experimenter based, random, or caused by some other uncontrolled variable is never examined. This still might be acceptable had not the authors argued at the end of the paper that they suffered from what they termed the "law of small numbers". I assume that means that with greater n they would have gotten their results. I object to that excuse for at least two reasons. First, they could have, with just a little better pre-experiment design, controlled for this. Second, sample sizes of twenty or better, drawn from the endpoints of a distribution should be sufficient to reveal any real differences. This is especially so with the literature on apprehension being as it is. Over fifteen studies have used similar procedures with this n or smaller in each cell and have discovered hypothesized differences.

The scales used by Scott, et. al. to measure satisfaction have no reported validity or reliabilities. Creating scales at whim may be fun, but empirically have little justification.

The test of the hypotheses (especially one through nine) could have better been done with any number of procedures. I will try to avoid any in-depth statistical analysis but a few things should be mentioned irregardless. First, these researchers opted to use independent t-tests nine seperate times. Alpha level was set at .05 with no regard to the number of tests being made, I would recommend a more conservative alpha derived via Bonferroni's method (eg., Miller, 1966). Alternative strategies for testing means would also have greater strength. For example, if the researchers want to stick with their format as is I would suggest they consider either a 3 x 2 analysis of variance with repeated measures with subsequent analyses for simple effects (Winer, 1972) or the mutlivariate analogue to the t-test, Nottelling's T^2 with appropriate subsequent tests (Morrison, 1967). Finally, given the relatively high correlation between different measures of attitude and satisfaction this data would be opportune for multivariate analyses of variances. What I'm suggesting here is that the approach taken to data analysis here is inappropriate given the nature of the data. The rationale, the hypotheses, and the data all suggest better alternatives.

A final note should be made about one procedure used in this paper to test some differences. When the authors discovered that the means in some categories were in the direction opposite of that predicted they chose a multiple comparison with known conservative bias-the S-method developed by Scheffe. I wonder through about the philosophical rationale for such a test. If an experimenter hypotheses one happening with strong rationale than how in the world is he ever going to interpret data going in the opposite direction. What I suggest would be that tests be made in accordance with the entry rationale and not with the data.

But if the authors of this report can justify such snooping I would question alternatively why they didn't continue their snooping one step further. To me, the most impottnat finding has little to do with apprehension; instead it deals with attitudes and satisfaction with mediated as opposed to traditional forms of instruction. The Scott, et. al. study shows clearly that subjects disliked technologically advanced instruction significantly more than the more traditional forms. Below I have computed the means of means for all three dependent variables. Allwere summed across apprehension level.

	Mediated	Written	Oral
Att. towards Meth.	16.315	20.405	20.065
Att. towards Cont.	16.445	20.795	20.055
Satisfaction	14.32	19.515	19.23

In every case the lowest two cells were those of the mediated form of instruction. An analysis employing analyses of variance techniques more fully than Scott, et. al. would have immediately found these differences. Summing across instructional format demonstrates that the differences due to apprehension were not as large. I would hope a reanalysis of the data would be completed.

	High	Low
Att. towards Method	18.3	19.56
Att. towards Cont.	18.25	19.95
Satisfaction	16.68	18.73

All in all I agree with the authors that more research needs to be done in this area. I think they present strong evidence for believing that new forms of education may need modification, or that student attitudes towards these forms need to be more noted.

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